

Comments on *Palaemon rosenbergii* De Man, 1879 (currently *Macrobrachium rosenbergii*; Crustacea, Decapoda): proposed conservation of usage by designation of a neotype

(Case 3428; see BNZ 65: 288–293; 66: 340–341)

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I fully support the application to conserve the name *Macrobrachium rosenbergii* for the commercially farmed freshwater prawn species we all know as *Macrobrachium rosenbergii dacqueti* (or *Macrobrachium dacqueti*) as proposed by Dr Daisy Wowor and Prof. Peter Ng. Keeping the name as such would avoid unnecessary confusion especially among aquaculture scientists and industry practitioners who have known the species as *M. rosenbergii* from scientific and popular publications. I together with other colleagues at SEAFDEC/AQD have been working on this commercial species as well as the other native Philippine species (proposed as *Macrobrachium wallacei*) to date and would like to be certain of the standard scientific names as we write about our research results in scientific publications.

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I hereby support Case 3428 to conserve the usage of the specific name of *Macrobrachium rosenbergii* (De Man, 1879) for all the reasons raised by the authors Daisy Wowor and Peter Ng.

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I am writing to express my support for the evidence as laid out in Case 3428 as well as the comments.

Comment on the proposed conservation of *Papilio danae* Fabricius, 1775 (currently *Colotis danae*; Insecta, Lepidoptera, PIERIDAE)
(Case 3488; see BZN 66: 250–255)

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This seems to me to be an open and shut case. The stability of nomenclature of a widespread, well-known and widely studied butterfly is jeopardised by the resurrection of an almost completely forgotten name that originally happened to be placed in the same catch-all genus, and that is itself generally considered to be a junior synonym of a species now placed in a different family. The chance of the two names being confused is remote. The authors make their case for the suppression of the senior name, and the conservation of the junior, carefully, logically and lucidly. I strongly support it.

The case raises a wider issue. It is this over zealous application of the rules, however well meant, that causes unnecessary changes to well-established names and threatens to bring those rules and formal taxonomy itself into disrepute among the many biologists that undertake taxonomy, but who are not steeped in the culture of the Code. If these biologists lose faith in the rules of nomenclature and start ignoring them, confusion is bound to follow. In my opinion, in the case of these ancient and temporary homonymies, stability of nomenclature should take precedence over the strict application of priority. In the present case, and I would guess most others, the two nominal taxa shared a common generic placement for a few decades until each was transferred to another, separate genus in a different family.

Clearly, when earlier names or usages are discovered, they should be reported, but the assumption should be that the stability of current nomenclature should be maintained. The rules may need to be changed so that only in cases of genuine confusion – a vanishingly small number, I should guess – would a case need to be made to the Commission to request the formal suppression or conservation of once homonymous names.

Comments on *Raja say* Le Sueur, 1817 (currently *Dasyatis say*; Chondrichthyes, Myliobatiformes, DASYATIDAE): proposed change of spelling to *Raja sayi* Le Sueur, 1817

(Case 3410; see BZN 65: 119–123).

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We, the members of the American Fisheries Society and American Society of Ichthyologists and Herpetologists joint Committee on Names of Fishes, write in opposition to pending Case 3410. The Bluntnose Stingray, *Dasyatis say* (Lesueur,

1817), is a widely distributed myliobatiform chondrichthyan occurring in the western Atlantic from the coast of the northern United States (Massachusetts) south to northern Argentina. The spelling of the specific name, *Raja say* Lesueur, 1817, is fully justified and in compliance with Article 31.1 of the present Code. The species was listed in three editions of *Common and Scientific Names of Fishes from the United States and Canada* (1960, 1970, 1980, American Fisheries Society Special Publications 2, 6, 12) as *Dasyatis sayi*. However, in the 5th edition (1991, Special Publication 20) the spelling was changed to *Dasyatis say* to conform to the original spelling as outlined in the 3rd Edition of the Code (1985; Article 31a). This spelling was also used in the 6th Edition (2004) of *Common and Scientific Names of Fishes from the United States, Canada and Mexico* and will again be used in the forthcoming 7th Edition (in preparation). In addition, at least ten other North American species of fishes have specific names formed as a noun in apposition (e.g. *Galeocerdo cuvier*, *Lophotus lacepede*, *Gobiosoma bosc*, etc.). These names are in prevailing usage and have been firmly entrenched in the North American ichthyological literature.

In our opinion, the original spelling has been the prevailing usage in North America for the last 18 years. Hence, we find Case 3410 to be counterproductive to Article 31.1 as well as to the spirit of the Code. We strongly favour retention of the original spelling following Article 31.1 and support prevailing usage. The continued use of the original spelling is in the best interest of stability and future uniformity.

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We have always been mildly irritated by the correct original spelling of *Raja say* and the confusion that the formation of the specific epithet generated, but we saw no justification for correcting it. However, since Santos and Carvalho (BZN 65: 119-123) have initiated this process we wish to support their petition.

Unfortunately, we cannot completely agree with their argument that *Dasyatis sayi* is the spelling in prevailing usage in terms of Article 33.2.3.1. The scientific literature contains many uses of *D. sayi* but also a number of uses of *D. say*. The *D. sayi* spelling predominated for many years in the scientific and particularly in the popular literature, but *D. say* was also in use. We do not expect that this will be sufficient to be considered prevailing usage however broadly that may be defined in the Code. The correct original spelling 'say' has generated numerous subsequent emendations to 'sayi' in the scientific and popular literature by well-meaning authors, who just assumed a taxonomist would have formed the name properly (Article 31.1.2; Recommendation 31A). Even if the Commission votes to uphold the original spelling, the confusion will continue. The only way to avoid confusion in this case is to emend the spelling 'say' to 'sayi'.

**Comment on the proposed conservation of *Pseudobagrus* Bleeker, 1858
(Osteichthyes, BAGRIDAE)
(Case 3455; see BZN 65: 202–204)**

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We are writing to comment on the petition of López et al. for the Commission to use its plenary power to suppress the siluriform generic name *Tachysurus* La Cepède 1803. The petition results from our publication clarifying the identity of *Tachysurus* (Ng & Kottelat, 2007). The consequences of our neotype designation, as described by López et al., are not new since we detailed all.

It has been more than a year since this application was published and we had hoped that this case would be important enough to elicit comments. The fact that no one has written to support or oppose the petition can be seen as an indication of the (un)importance of the case. Since no comments have been forthcoming, we explain below why we do not see the use of *Tachysurus* instead of *Pseudobagrus* as a threat to stability of nomenclature. Once considered in the context of nomenclature of Asian freshwater fishes, the changes are much less dramatic than suggested by López et al.

La Cepède (1803) described *Tachysurus sinensis* from a Chinese painting. The generic name *Tachysurus* remained unused until Eigenmann & Eigenmann (1888) first applied it to the predominantly marine catfishes of the family ARIIDAE. *Tachysurus* was used for members of the ARIIDAE for about 30 years (as a senior subjective synonym of *Arius* Valenciennes, 1840) until Jordan (1923) raised the possibility that *T. sinensis* could refer to a freshwater catfish and not an ariid. Since then, the usage of *Tachysurus* for members of the ARIIDAE has been more sporadic, with both *Arius* (e.g. Inger & Chin, 1962) and *Tachysurus* (e.g. Smith, 1945) being used. Wheeler & Baddokwaya (1981) presented evidence that *T. sinensis* was not a member of the ARIIDAE; since then the usage of *Tachysurus* for ariids has sharply decreased and is now restricted to non-taxonomic literature (see below).

Referring to Wheeler & Baddokwaya (1981), López et al. (BZN 65: 202–204) commented that ‘the identity of *Tachysurus* remained ambiguous as an undetermined siluriform’ until our designation of a neotype for *T. sinensis* (in passing, admitting that we solved the problem). This is not exactly the case. Wheeler & Baddokwaya (1981) addressed only the problem of whether *T. sinensis* was an ariid or not, and having concluded that it was not, they did not bother to explore its actual identity. With experience of the East Asian Siluriformes, they would have realised that the figured fish can only belong to the Bagridae.

That *T. sinensis* was not recognised as a bagrid for a long period was probably largely due to the lack of access to the figure in the original description. The figure (reproduced by Wheeler & Baddokwaya, 1981), shows several features (e.g. color

pattern) clearly attributable to the bagrid catfish genus then recognised as *Pseudobagrus*. Anyway, the name *T. sinensis* remained in limbo, variously listed as nomen dubium or incertae sedis. Nomina dubia and incertae sedes are not meant to be permanent statuses: their fate is to become valid or invalid. They remain potential threats for younger valid names and for the sake of stability their identity must be resolved, the sooner the better. In that aspect *Tachysurus* is a good example. Had the identity of *T. sinensis* been clarified in 1923, it would quickly have reached general usage. Nobody tried to resolve the taxonomic and nomenclatural status of *T. sinensis* until our addressing the problem and solving it by a neotype designation (Ng & Kottelat, 2007).

The general misapplication of the name has stopped since Wheeler & Baddokwaya's (1981) identification of *T. sinensis* as a catfish not of the family ARIIDAE. Although the name *Tachysurus* is still occasionally and mistakenly applied to members of the ARIIDAE, a search of the Zoological Records Online (conducted on 3 August 2009) for the period 1981–2009 revealed that *Tachysurus* has been used for members of the ARIIDAE only ten times within the last ten years, and always by fisheries scientists or parasitologists from one country (India). Fisheries literature notoriously may ignore taxonomic and nomenclatural changes for dozens of years. Should ill will, lack of information or incompetence be used as a standard to decide on validity of names? Given the small number of incidences and its restriction to users from a single country, we feel that this is unlikely to lead to widespread confusion should *Tachysurus* remain in use for East Asian bagrid catfishes.

The present generic nomenclature within the Bagridae dates from Jayaram (1968), who organised East Asian bagrids in five genera (*Bagroides*, *Coreobagrus*, *Leiocassis*, *Pelteobagrus*, and *Pseudobagrus*). Mo (1991) showed that the East Asian species that Jayaram assigned to *Bagroides* and *Leiocassis* were not congeneric with those from Southeast Asia and Mo assigned them to either *Pelteobagrus* or *Pseudobagrus*. However, Chinese authors still persist in using *Leiocassis* for some East Asian species (e.g. Zheng & Dai, 1999; Yu et al., 2009). The assignments of some species keep shifting between *Pseudobagrus*, *Pelteobagrus* and *Leiocassis*. The notion of a stable *Pseudobagrus* as presented by López et al. became so only when Ng & Freyhof (2007) placed all of the East Asian taxa into a single genus (*Pseudobagrus*), citing previously published morphological and molecular evidence. These facts greatly weaken the argument that conservation of *Pseudobagrus* would save us from 'taxonomic confusion'.

López et al. contend that the usage of *Pseudobagrus* for the East Asian members of the Bagridae is widespread, and cite usage in at least 135 papers in 50 years as an example. This approximates to three papers a year, a very low rate for a genus within a 'group that . . . includes species of commercial significance' and with a large body of literature. The proposal by López et al. compares unfavorably with the change of both the generic and specific names of the rainbow trout from *Salmo gairdneri* to *Oncorhynchus mykiss*, which did not cause any significant problem to users and became established very quickly. The usage of the binomen for the rainbow trout is considerably more extensive, being cited each year in thousands of scientific, technical, commercial and popular publications, and mentioned in national and international legal instruments. The species is the object of a trade worth billions of dollars annually.

The list of 135 references that López et al. provide includes many publications in Chinese journals, giving the impression of a common usage of the name *Pseudobagrus*. The reality is somewhat different. Our experience with Chinese journals is that binomens are used only in the title, introduction, and/or abstract (and sometimes in tables and figure/table captions; the latter case only occurs when there are English translations of the captions) and it is the Chinese name for the species that is used throughout the text. It was not possible for us to verify if this is true for all of the non-taxonomic papers in the listed Chinese journals simply because we could not access them, but it is confirmed in those we could obtain. Also, we do not have the luxury of investing days in what we consider a sterile diversion from more important taxonomic research; we do understand, however, that others may not share our priorities.

As explained in our paper (Ng & Kottelat, 2007), we have been aware of the nomenclatural problems surrounding *Tachysurus* for a long time. That the problem has been mentioned in publications published between Wheeler & Baddokwaya's (1981) study and our neotype designation (e.g. Kailola, 2004) indicates that other ichthyologists are also aware of it. The resolution of the identity of *T. sinensis* was made necessary by a checklist of freshwater fishes of southeastern Asia being prepared by one of us (MK). This led to the discovery of (and need to make decisions on) about 25 cases of genera of uncertain identity and/or presenting priority conflicts leading to name changes. It was recognised that most of these name changes would be annoying but could be avoided only by applications to the Commission. That so many nomenclatural problems subsist at the genus level, even in taxonomic literature, indicates how far we are from a stable nomenclature in the covered group and area. Besides, these are changes for strictly nomenclatural reasons, not reflecting taxonomic problems; we expect that many more changes at genus level will result from future taxonomic research. It was decided that submitting about 25 applications (about equal to the number of applications published in the 2009 volume of BZN!) would make less sense than submitting applications only for cases of great complexity or ones in which family group names were involved. (e.g. the *Mystus* case, which would have meant changing the names of two families; see BZN 64: 100–102 and Opinion 2209, BZN 65: 237–238).

Regardless of the consequences, the *Tachysurus* case is quite trivial. There were two possible solutions: (1) designation of a neotype or (2) suppression of the names under the plenary power of the Commission. Our decision to designate a neotype for *T. sinensis* was taken after consultations with colleagues. The neotype designation allows an immediate decision without involving the Commission and the resulting delays, printing costs, work load, etc.; this is the solution we chose. We regret that the application by López et al. has now postponed a stabilisation of the names for this genus by several years and generated expense and work. Our decision was bolstered by the relative unimportance of the case, as discussed above.

We wish to comment on one of López et al.'s concluding sentences that 'the original description of *T. sinensis* ... is unlikely ever to yield a satisfactory association with a recognised group'. That an original description be deficient is not a problem in itself. The association of a name to a taxon is made by the type, not by the description, and this was exactly the purpose of the neotype designation that we made.

In conclusion, we suggest that the Commission should not use its plenary power for this relatively minor case, which has been unambiguously cleared by the simple application of the Code.

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Comments on the proposed conservation of usage of *Testudo gigantea* Schweigger, 1812 (currently *Geochelone (Aldabrachelys) gigantea*) (Reptilia, Testudines)
(Case 3463; see BZN **66**: 34–50, 80–87, 169–186; 274–290; 352–357)

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I am writing in support of Case 3463. I am a non-specialist in this field and as such I found the taxonomy of the Indian Ocean giant tortoises to be complicated and confusing when researching the topic for my book on these animals, *A Sheltered Life* (2004). Based on a literature survey and interviews with researchers, I concluded that *Geochelone (Aldabrachelys) gigantea* was the most appropriate name for this important and iconic species. However, after the book's publication I received correspondence which opposed my use of this name. As an interested observer, I am perturbed by the continuing uncertainty connected with the naming of the Aldabra tortoise and so I strongly support Dr Frazier in his desire to settle this nomenclatural issue.

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Jean-Jacques Dussumier and Aldabra

Pat Matyot (2009) has raised interesting but delicate questions in connection with the lectotype of *Testudo dussumieri* Gray, 1831. Did Dussumier really travel to Aldabra, and therefore is this specimen really an Aldabra tortoise? First, we could extend the questions such as ‘is the present Aldabra the same island as that so named in the 18–19th centuries?’ (see e.g. Devaux 2006, p. 30, about the Aldabra map by Picault & Grossin, reproduced by Günther (1877), which actually shows the present Farquhar), or even ‘did Dussumier really exist?’ Laissus, his only known biographer, begins his notice with the words ‘La vie de Jean-Jacques Dussumier n’est que très imparfaitement connue et seulement dans ses épisodes principaux’ [‘Jean-Jacques Dussumier’s life is very imperfectly known and then only during his main episodes’] (Laissus, 1973, p. 387).

Next, it must be emphasised that at the time of Dussumier’s travels, Aldabra was already well known for its tortoises, and these tortoises were regularly brought to (inter alia) La Réunion island – sailors did not wait until the formal possession of Aldabra in 1892 – as reported in the local newspaper *La Feuille hebdomadaire de Bourbon*, for instance n°628 (12.01.1831): ‘Belles tortues d’Aldabra à vendre à des prix modérés. S’adresser rue du Barachois . . .’ [‘Beautiful Aldabra tortoises for sale at moderate prices. Address rue du Barachois . . .’] (Bour, 1981, p. 122). If Dussumier did not really go to Aldabra, he could have intercepted a ship with a load of tortoises close to the island, or even bought some genuine specimens on Réunion or elsewhere. But could we really prove that Dussumier never landed on Aldabra?

We hardly understand the comments by Matyot about the correctness or completeness of Dussumier’s collecting data. Dussumier himself wrote in 1830: ‘Pour tous [fish specimens] j’ai eu soin de tenir un registre, où j’ai inscrit, au numéro correspondant à celui que porte chaque individu, les couleurs qu’il avait au moment où il a été pêché, et j’y ai joint les renseignements que j’ai pu me procurer, après en avoir vérifié l’exactitude’ [‘For all [fish specimens] I have been careful to keep a register where I have noted, against the number corresponding to that carried by each individual, the colours it possessed at the time it was caught, and I have added such bits of information as I have been able to get, having checked their accuracy’] (Laissus, 1973, p. 392). Such care was highly appreciated by the exacting Cuvier, and

Matyot's comments outlining that 'Gray was tempting men to steal and sell him their specimens' are irrelevant. In the present case, Gray just saw (in 1829) the specimen in the Leiden Museum, and at that time he had no connection with either the Paris Museum or its associated sellers 'on the sly' or with Dussumier himself. On the other hand, as he did with other observed specimens, Gray approximately copied and then published in 1831 the data written on the tag, in this case Dussumiere [sic] as collector and Aldebra [sic] as locality, and we see no reason to doubt their accuracy. As pointed out by Matyot himself, fifty years later Hubrecht (1881), at that time curator of fishes at the Leiden Museum, repeated the same data. The specimen had been labelled by Hermann Schlegel, also a former curator, who later described the Round Island Boa (Schlegel, 1837) and dedicated it to its collector, Dussumier, 'voyageur infatigable et ami éclairé des sciences' ('tireless traveller and enlightened friend of Science').

Finally, Matyot (2009, p. 353) mixed up two distinct young specimens collected by Dussumier, one true (granitic) Seychelles tortoise, seen by Duméril & Bibron, still in the Paris Museum (MNHN 1942; Bour, 2006) and the Aldabra tortoise, seen by Gray, still in the Leiden Museum (RMNH 3231). Dussumier, himself, clearly separated them, on morphological as well as on geographical bases, but it is likely that Matyot never saw either one.

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(3) Roger Bour, Peter C. H. Pritchard and John B. Iverson

(addresses as above)

The present statement is proposed in order to establish definitely on which bases the valid scientific name of the Aldabra tortoise rests. We specifically address the five major points of contention which were disputed in the previous comments about this case published in this journal (BZN, **66**(1), **66**(2) and **66**(3)). We claim to be professional taxonomists of some experience and we aspire to take into account only factual, objective data. The articles and recommendations of the International Code of Zoological Nomenclature presently in force, here the 'Code', are followed. The colleagues who claim they refuse to make the adjustment of giving up the name *gigantea* for the Aldabra tortoise need to agree that, for 200 years, many changes were implemented in the phylogenetic and taxonomic appraisal of the group of tortoises at stake. 'Comfort levels' are not pertinent to the case at hand, and emotion and personal preference are not the criteria by which the case should be judged.

The quality of Schweigger's diagnoses and descriptions is poor

The work of Schweigger (1812) is based on actual specimens, and includes measurements for the new species. It is without any doubt the best of its epoch. At that time, other similar works were mainly founded on previous publications and on more or less literary sources. If Schweigger's descriptions are deemed inadequate, then all descriptions prior to at least Duméril & Bibron (1835) could be called into question. Schweigger mainly worked with the collections of the Paris Museum (MNHN), and presently the type specimens of 14 distinct species described by this author have been identified in this institution, thanks to the accuracy of the publication (see Bour, 2008b). We wonder who among the authors of the published comments have directly read and/or translated the original Latin description, and who among those who have questioned the rediscovery have actually seen the types of any of these early names. Unfortunately, this case is not an isolated one. We predict that future morphological and molecular analysis of type material will show that many names have been misapplied or misunderstood in the past.

The holotype of *Testudo gigantea* Schweigger, 1812 was 'lost' for nearly two centuries

In fact this holotype was misplaced and wrongly identified for a long time. The specimen was also studied and measured by Duméril & Bibron (1835), who did not recognise it as being the 'type' of this species. A careful reading of their opus reveals several other similar misinterpretations, but such an exposition is beyond this note. The recent rediscovery of the type of *T. gigantea* in the MNHN is used by some as argument against our proposition, but we will not debate the defamatory insinuation of a timely rediscovery, like 'rediscovered just when it was supportive for their point of view'. Actually this is not unusual, especially given that few modern chelonian researchers deem the examination of type material as essential to systematic study. We can give several references of recent 'rediscoveries' of type specimens: Iverson & McCord (1989) for *Emys muticus* Cantor, 1842; Pritchard (1996) for *Testudo ephippium* Günther, 1875; Bour & Maran (1998) for *Emys leprosa* Schweigger, 1812; Bour (2008a) for *Testudo angulata* Schweigger, 1812; Rhodin & Carr (2009) for *Testudo scripta* Schoepff, 1792. In some cases, these discoveries challenged the 'accepted' nomenclature: that is science and science recognises change; if not, it is no longer science, it is dogma.

This is not the holotype of *Testudo gigantea*

Bour (2006) has extensively studied each of the details provided in the description(s) of *Testudo gigantea* Schweigger, 1812, which led to the recognition that MNHN 9554, long identified as *Chelonoidis denticulata* (Linnaeus, 1766), a yellow-foot tortoise, is undoubtedly the holotype. But who really understands the diagnostic value of 'testa cylindracea . . . pedes squamis robustis, latissimis robusti . . . marginis viginti tres, aequalis'? Actually, among all known chelonians, a large and elongated tortoise with large scutes on the forearms, no cervical (nuchal) scute, and rounded flanks could only apply alternatively to a Mascarene tortoise (genus *Cylindraspis*); but the origin of the specimen (Brazil, via Lisbon) removes any doubt. It must be explained here that the MNHN collections have gone through a period of disorganisation from 1965 to the opening in 1994 of the 'Zoothèque', an underground storage

building. Several comments allude to a deficient original description, but none of them has provided any precise character that would support a refutation of the identity of the type of *T. gigantea*; they have simply rejected our interpretation. Moreover, no person among the detractors has asked either to examine the specimen, nor for pictures of it. Finally, we will just mention again one measurement: the holotype of *Testudo gigantea* had a curved length of 767 mm according to Schweigger; the large specimen of *C. denticulata* measured by Duméril & Bibron had a curved length of 770 mm; and the 'rediscovered' specimen of *C. denticulata* (MNHN 9554) has a curved length of 770 mm. Sometimes numbers are more eloquent than a long description.

The neotype designation is the solution

We here strictly quote the Code, precisely the relevant parts of Article 75.3. A valid neotype is designated when the designation is published with the following particulars:

'75.3.4. the author's reasons for believing the name-bearing type specimen(s) (i.e. holotype, or lectotype, or all syntypes, or prior neotype) to be lost or destroyed, and the steps that had been taken to trace it or them'. Such mandatory statements are completely wanting in Frazier's (2006a) paper.

'75.3.5. evidence that the neotype is consistent with what is known of the former name-bearing type from the original description and from other sources'. This point was already raised by Bour (2006), who noted these differences between the neotype and the holotype: 'e.g. absence vs. presence of a cervical scute; limbs shielded by tough and very broad scales vs. only postcranial skeleton, and fragments of skin'; we can also add: a full specimen (head, limbs, tail are described) vs. a shell.

'75.3.6. evidence that the neotype came as nearly as practicable from the original type locality [Art. 76.1] and, where relevant, from the same geological horizon or host species as the original name-bearing type'. The neotype came from Aldabra, the holotype from Brasil, unless someone can demonstrate that the latter is wrong.

'Recommendation 75B. Consultation with specialists. Before designating a neotype, an author should be satisfied that the proposed designation does not arouse serious objection from other specialists in the group in question'. Nothing was submitted at least to the present authors or to Justin Gerlach, who are among the taxonomists who recently published the most extensively on the systematics of these tortoises.

Finally, according to Article 75.8, a holotype always out-trumps a neotype: 'If, after the designation of a neotype, the name-bearing type (holotype, syntypes, lectotype or previous neotype) of the nominal species-group taxon that was (were) presumed lost is (are) found still to exist, on publication of that discovery the rediscovered material again becomes the name-bearing type and the neotype is set aside . . .'. The only conclusion is that the neotype designation is both unnecessary and unacceptable.

The stability stands with the name *T. gigantea* Schweigger, 1812

Beside the rebuttal of the identity of the type specimen of *T. gigantea*, a major expressed argument is the stability of the name in connection with the Aldabra

tortoise. First, we note that the most recent (1982–2006) 36 names (nominal combinations) for the Aldabra tortoise, as listed by Fritz and Havaš (2007), only include a single combination that contains the word *gigantea*. Second, in order to have an idea about the respective importance of the main combinations used to name the Aldabra tortoise, and the involved genera, we used ‘Google’ data, i.e. a procedure which can be reproduced by anyone, but whose results may be very volatile, as developed below. We limited our research to the species *T. gigantea* Schweigger, 1812, *T. dussumieri* Gray, 1831, and *T. elephantina* Duméril & Bibron, 1835. The results (updated on 22 October 2009) are the following:

Combination used	Occurrence	% of subset
<i>Testudo gigantea</i>	8130	85.1
<i>Testudo elephantina</i>	1290	13.5
<i>Testudo dussumieri</i>	136	1.4
<i>Geochelone gigantea</i>	12500	91.6
<i>Geochelone elephantina</i>	71	0.5
<i>Geochelone dussumieri</i>	1080	7.9
<i>Aldabrachelys gigantea</i>	4130	38.5
<i>Aldabrachelys elephantina</i>	6360	59.3
<i>Aldabrachelys dussumieri</i>	234	2.2
<i>Dipsochelys gigantea</i>	255	1.3
<i>Dipsochelys elephantina</i>	2250	11.2
<i>Dipsochelys dussumieri</i>	17600	87.5
<i>Aldabrachelys</i> (alone or combined)	13700	16.8
<i>Dipsochelys</i> (alone or combined)	67900	83.2
TOTAL <i>gigantea</i>	25015	46.3
TOTAL <i>elephantina</i>	9971	18.5
TOTAL <i>dussumieri</i>	19050	35.3

These data reveal that an agreement on usage for the species or the genus names does not exist. Actually these figures are continually moving and they obviously reflect a drift, rather than providing an absolute amount. On 1 August 2009 the number of returns for *Aldabrachelys* was 9510, for *Dipsochelys* 88100; as shown above, on 22 October (when this paper was submitted for publication) this number was 13700 for *Aldabrachelys*, 67900 for *Dipsochelys*; finally, on 28 December (a corrective was sent to the editor) this number was 23200 for *Aldabrachelys*, decreasing to 52700 for *Dipsochelys*. Moreover, within two months some references have overwhelmingly increased: *Testudo elephantina* from 1290 to 5300, *Testudo dussumieri* from 136 to 13800, *Testudo gigantea* from 8130 to 91200! These figures greatly confirm that ‘Google’ numerical data, if applied for more than a brief period, should be used and interpreted with caution, as already underlined by Dubois (2007) and repeated by Frost et al. (2009). In fact, it could be argued that only figures obtained prior to the raising of the case by Frazier (2006a) should be considered, as they were not biased by the debate (see in this respect Dubois, 1997: 319). We must also note that the title of the Case in the ICZN Bulletin was itself biased, including ‘Currently *Geochelone (Aldabrachelys) gigantea*’, despite a request by one of us (PCHP). This combination only returned 76 ‘hits’.

Presently, all mentions of *gigantea* (including *Testudo gigantea*, which is obviously an outdated combination, used by non-taxonomists) added together show that it is

the most used species epithet by only a moderate margin (46.3% vs. 35.3% for *dussumieri*). On the other hand, the most used combination is *Dipsochelys dussumieri*: 17600 returns vs. 12500 for *Geochelone gigantea* (58.5% vs. 41.5%). However, it is clearly evident that there is no consensus, no established name, i.e., there is not current stability, contrary to the allegations of many comments and of the title of the Case. It simply cannot be argued that the current name is *gigantea*. Furthermore, the only numerically significant observation is the predominance of *Dipsochelys* over *Aldabrachelys* (83.2% vs. 16.8%).

The ‘Code’ must not be taken apart; it must be understood, accepted and followed

Should zoological nomenclature be regulated by a set of rules or by ‘polls’ open to anyone, even without any experience in taxonomy? If so, then the easiest way would simply be to get rid of the Code, and let so-called ‘consensus’ establish the valid names of taxa. Experience in the past has amply shown that consensus rarely ever leads to stability and clarity in the use of names, and often leads to chaos, which is precisely why a Code had to be established in the late 19th century. We believe that dismantling the Code in favor of common opinion would be a mistake.

Finally, why should we reject the name *Testudo dussumieri*, which honours the memory of Jean-Jacques Dussumier, the first traveller who brought back an Aldabra tortoise with its precise locality and offered it to science? If one operates by the letter of the law (Code), as we have, and not by passion or emotion, it is clear that the first valid name for the Aldabra tortoise is *Testudo dussumieri*.

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(4) Gregory B. Pauly

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I am writing in support of the proposal described in Case 3463 to conserve the name *Testudo gigantea* Schweigger, 1812 for the Aldabra tortoise by maintaining a recently designated neotype and suppressing the name *Testudo dussumieri*. Such an action would alleviate any recent confusion and nomenclatural instability that might have arisen regarding the most appropriate name. This confusion stems from the uncertain species identity and collection locality of the specimen used in Schweigger's description; this specimen lacked individual identification (e.g., an accession number) and was only listed as being 'in the Paris Museum.' Recognising that the type specimen was unknown, Frazier (2006) designated a neotype for *Testudo gigantea* to stabilise the nomenclature of the Aldabra tortoise. Subsequently, Bour (2006) claimed to have found Schweigger's lost specimen (MNHN 9554), which he identified as the South American taxon *Chelonoidis denticulata*. However, there remains uncertainty as to whether MNHN 9554 is the specimen examined by Schweigger, in part because this would require misidentifications by Dumeril and Bibron (1835) who were in contact with Schweigger and who specifically stated that *T. gigantea* of Schweigger was not the species that we now call *C. denticulata*. Given this uncertainty, taxonomic debates and nomenclatural instability will continue for the Aldabra tortoise without action by the ICZN. Further, even if MNHN 9554 could unquestionably be identified as Schweigger's tortoise specimen, under Article 75.6 prevailing usage should be conserved by designation of a neotype. To promote stability and universality, I support maintaining USNM 269962 as the name-bearing neotype. A significant advantage of this approach is that it results in continuity of usage of the nomen *gigantea*, which has been used for over a century in numerous scientific and non-scientific writings to reference the extant tortoises from Aldabra Atoll.

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I too support the application of Frazier to conserve *Geochelone gigantea* Schweigger, 1812 by accepting his neotype designation. As recently as 2004, my colleagues and I (Joyce et al., 2004) used the term *Dipsochelys dussumieri* (Gray, 1831) to refer to the Aldabran giant tortoise. However, Frazier compellingly illustrates that usage of this term has resulted in substantial nomenclatural instability. Much of the problem is ultimately based on the highly confusing type situation. I thus fully agree that stability is best served by accepting Frazier's neotype and by conserving the name that has been used most consistently for the last 75 years, i.e. *Geochelone gigantea*.

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The remarkable and impassioned correspondence on the name of the Aldabra tortoise has thrown up some information that requires further notes to supplement my earlier submission (BZN 66: 174–176).

1. Matyot (BZN 66: 352–354) has offered evidence to suggest that it is unlikely that Dussumier collected specimen RMNH 3231, designated as lectotype of *Testudo dussumieri* by Bour (2006b), on Aldabra. Although many details of his travels are unclear (see Laissus, 1973), I am inclined to agree that Dussumier did not visit Aldabra, but this does not mean the animal did not originate there, nor does it invalidate the name under the Rules. At that time, when native tortoises in the granitic Seychelles were so reduced that recorded export to Mauritius had ceased (Toussaint, 1967; Stoddart & Peake, 1979), from 1815 the principal source of tortoises for human consumption on Mauritius and the granitic Seychelles was Aldabra (Mondini, 1990). Fairfax Moresby (1842, p.741), writing in 1822, reported 'Aldabra is annually visited in the favourable monsoons for the land-tortoise, which are to be found most plentifully. They grow to a large size, are taken to Mahé or the Mauritius, and sold from one to three Spanish dollars each'. Some of those taken to the Seychelles were then re-exported to Mauritius. Théodore Sauzier (1893) (also Stoddart, 1971, Stoddart & Peake, 1979) cited import from the Seychelles of 3400 tortoises into Mauritius in 1826 alone. The origin of 2600 he presumed to be Aldabra on the grounds that there were no longer such numbers in the granitics; the other 800 came directly from Providence. A handwritten footnote by Sauzier in his own copy of Sauzier (1893) uses further manuscripts to raise the 1826 import total to 4800: 4000 on ships incoming from the Seychelles, 800 direct from Providence (copy in the Radcliffe Science Library, Oxford). It should be added that, as George Harrison, the lessee of Providence, was also government agent in the Seychelles, the Providence shipment could also have originated on Aldabra. Stoddart & Peake (1979) doubted the aboriginal existence of tortoises on Providence and suggested this shipment reported by Sauzier may have been marine turtle, but the number seems excessive for a single haul of *Chelonia mydas*. Slave ships used the excuse of visiting Aldabra for tortoises to cover their illegal trade (Scarr, 2000), also actually collecting tortoises to maintain the story. The slavers were often trafficking to Réunion (McAteer, 2000), and Bour (1981) reported Aldabra tortoises advertised for sale there in 1831 and 1834. Dussumier visited all these islands regularly (Laissus, 1973; Bour, 2006a; Matyot, BZN 66: 352–354), and could easily have obtained an Aldabra tortoise on any one of them. He is known to have been in Mauritius in 1825 or early 1826 (Bélanger, 1834) and visited the Seychelles on the same voyage, which ended in spring 1826 (Laissus 1973) and not spring 1825 as reported by Bour (2006a). Dussumier was back in the Seychelles in 1827 and again in April 1828, then back and forth in the Indian Ocean with stops at Mauritius, Réunion and the Seychelles between 1828 and 1830, before returning to France in September that year. Paris Museum records show he deposited, inter alia, 11 reptile specimens and 13 live tortoises in November 1827, and 11 chelonians in 1830 (Laissus, 1973), though the last lot was too late to have included the *T. dussumieri* lectotype in Leiden, which Gray saw on his visit in 1829 (Bour, 2006a). As an assiduous collector for the Paris museum he would have ascertained its origin, and it is clear from

the citations earlier that the provenance of tortoises was well known in the islands. Matyot (BZN 66: 352–354) argues it would be atypical of Dussumier to collect ‘only a young tortoise and no other specimen from Aldabra’, but given the ubiquity of living Aldabra tortoises on the Indian Ocean islands he visited, he probably assumed they were well enough known as adults, and thus brought back only a juvenile. As Bour (2006a) has enumerated, he also brought adults of the Aldabran form back from Anjouan and native forms from the granitic Seychelles. Matyot further speculates that because of some possible shenanigans in the way Gray acquired specimens, the origin of this specimen should be disregarded, but a) it was not acquired by Gray for the British Museum (now NHM), but held in Leiden, and b) why invent the then extremely obscure (to Europeans) locality of Aldabra if there was no reason to do so? Irrespective of where the specimen was collected, it has been identified by all who have studied its morphology as a juvenile Aldabra tortoise (see photos in Gerlach, 2004a; Bour, 2006b) and has been shown by mtDNA analysis to be an Aldabra-Seychelles tortoise (Austin et al., 2003); hence it remains a valid lectotype for *Testudo dussumieri*. The only other possible origin of Dussumier’s specimen RMNH 3231 would be a native granitic Seychelles tortoise, rare but not extinct in the mid-1820s, of which Dussumier also brought back a juvenile (Bour, 2006a). Although the juveniles are similar (as is their DNA), they are morphologically distinguishable (Bour, 2006a; Gerlach, 2004a), and in any case the granitic Seychelles forms are generally considered (Austin et al., 2003, Palkovacs et al., 2003, Rhodin et al., 2009) to be conspecific with those on Aldabra; even Gerlach (2004a), while treating them as species, conceded that they were probably only subspecifically distinct. Hence *dussumieri* is the earliest valid name for the species as a whole if one accepts that the specimen of *Chelonoidis denticulata* MNHN 9554 is the rediscovered type of *Testudo gigantea* (Bour, 2006b; Bour & Pritchard, BZN 66: 169–174). Those who doubt this identification appear to do so on very weak grounds.

2. It is ironic that the holotype of *Testudo gigantea* turns out to be a *Chelonoidis*, for this brings out an interesting contrast in perception amongst those concerned with tortoises. Until recently the Galapagos tortoise complex, even more iconic and endangered than those on Aldabra, was generally known as *Geochelone elephantopus*, but has morphed in recent decades into *Chelonoidis nigra*, apparently without any of the arguments surrounding the nomenclature of the Aldabran animals—no cases or discussion in the BZN, or other controversy that I can locate. As Bour (2006b) pointed out: ‘Following Pritchard (mainly 1996), among some other changes, the universally used *Testudo elephantopus* Harlan 1827, a name for the Galápagos tortoises, was replaced by *Testudo nigra* Quoy & Gaimard 1824, apparently without major objection from scientists. On the other hand, Frazier (2006) strongly emphasised the general instability and chaos regarding the valid name of the Aldabra tortoise’. Crumly (1982), using morphology, drew attention to the apparent polyphyly of the broad genus *Geochelone*, and Pritchard (1984) first drew attention to the fact that *Testudo nigra* Quoy & Gaimard, 1824 pre-dated *T. elephantopus* Harlan, 1827. Even before Crumly’s conclusions were amply confirmed by DNA studies (e.g. Le et al., 2006), the use of *Chelonoidis* Fitzinger, 1835 had become frequent, and that of *nigra* almost universal. However, the IUCN’s red list still uses *Geochelone* (www.iucnredlist.org/apps/redlist/details/9011/0; accessed online 30/12/2009) despite the SSC’s Tortoise & Freshwater Turtle Specialist Group (Rhodin et al., 2008; 2009) using *Chelonoidis* in their world checklist. CITES used *Geochelone nigra* as early as

1999 without complaint (Charette & Gallegos, 1999). Although the retention of *Geochelone* in the IUCN red list may indicate residual conservationist resistance to name change, there seems to have been no suggestion that changing the Galapagos tortoise nomenclature was going to destabilise conservation measures or otherwise cause irretrievable chaos and misunderstandings as claimed by the proponents of Frazier's case. It is hard to avoid concluding that issues beyond mere science and nomenclature are at issue here.

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I find Pat Maytot's refutation of Marinus Hoogmoed's arguments compelling. I see enough reasons to retain stability (*gigantea*) and no justification to deviate from this.

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We note that despite the extensive discussions and opinions on this issue, one rare point of general agreement seems to be to accept that there is only a single species of 'giant land tortoise' on Aldabra Island, so the problem simply amounts to knowing what it should be called. If we include the original application by Frazier, in 2009 no fewer than 91 persons commented so far on this case in 72 contributions in four instalments of this Bulletin, covering 66 pages. We note that 85 of the intervening parties were in favour of the name *Testudo gigantea* for this species and 6 were in favour of the name *Testudo dussumieri*. However, we are convinced that nomenclatural decisions by the Commission should not rely on polls or on persons of variable expertise and insight, nor on campaigns seeking to gather supporters to form a 'pressure group', but should be based upon due consideration of explicit arguments, even if expressed by a 'minority' of stakeholders.

As the present curator (AO) and the previous two curators (AD, ERB) of the herpetological collection of the Muséum National d'Histoire Naturelle (Paris), one of the oldest and most important herpetological collections in the world, we agree with Gerlach's statement that the comments in favour of each name for the Aldabra tortoise rest on (1) taxonomic arguments regarding the identifications of the type specimens; (2) arguments for 'nomenclatural stability', relating to *usage* of names, irrespective of the identification of the taxa; (3) arguments concerned with the *conservation biology* of these tortoises and (4) arguments relating to the *appropriateness* of each name for the taxon. Let us consider these four sets of arguments in the reverse order.

Name appropriateness

Gerlach and Hansen discussed the respective appropriateness of the names *gigantea* and *dussumieri*, and *Aldabrachelys* and *Dipsochelys*. This discussion is totally irrelevant to the present issue. As pointed out by Dubois & Raffaëlli (2009, p. 18), scientific names are not descriptions, diagnoses, statements of characters, distribution or other characterisations of the taxa they designate, nor models or theories about the hypothesised origin of these taxa, nor praise for their authors, for the discoverers of the taxa or for the persons to whom they may be dedicated. They are just neutral *labels* designating unambiguously and universally given taxa within the frame of a given taxonomy, i.e. allowing automatic reference to the taxa recognised by taxonomists at a given stage of their research. These labels allow storage and retrieval of the information accumulated in taxonomies, but it is not mandatory that the names have any meaning at all. In many cases, factually true information about coloration, body form or geographic range may indeed be encapsulated in either or both of the two terms of a binomen, but in many other cases the name provides misleading statements about the taxon. This is why the Code expressly states that availability (and consequently validity) of names 'is not affected by inappropriateness' (Article 18), and allows a new generic or specific name to be 'empty of meaning', for example for being 'an arbitrary combination of letters provided this is formed to

be used as a word' (Article 11.3). If it were not so, thousands of zoological names would have to be changed. There is no point in further discussing the question of appropriateness of names in the present case.

Conservation biology

This was referred to by 42 of the 85 (49.4 %) supporters of the application, but, as correctly stated by Gerlach (BZN 66: 184–186, June 2009), it is not convincing: 'the tortoises referred to are explicitly the Aldabra tortoises, for which there is no significant identification issue whatever name is applied'. Whenever a decision is made regarding the valid name of this species, it will be a trivial matter to incorporate it in official checklists and documents, as rightly stressed by Hoogmoed (BZN 66: 354–356): 'international bodies like CITES are able to change names of species on their lists with few problems and without jeopardising the protection of the taxa in question. And the same holds true for governments and their agencies'. Such changes of names of taxa on official lists have already occurred on several occasions, without causing any problem for the conservation policy of the taxa concerned. As long as the species is well identified, a unique and universal name is not essential for the conservation of threatened taxa. Whereas the Aldabra tortoise is in Annex 2 of the CITES list, the following three taxa are in Annex 1 and their names, long considered to be 'universal', have changed during the last 50 years: the mammals *Papio sphinx* (Linnaeus, 1758) and *Gazella dama* (Pallas, 1766) have become respectively *Mandrillus sphinx* and *Nanger dama*; the bird *Diomedea albatrus* Pallas, 1769 changed into *Phoebastria albatrus*; the fish *Pangasionodon gigas* Chevey, 1931 is now known as *Pangasius gigas*; and the chelonians *Kachuga tecta* (Gray, 1831) and *Testudo elephantopus* Harlan, 1827 are now on the list respectively as *Pangshura tecta* and *Chelonoidis nigra* (Quoy & Gaimard, 1824). The last example is very similar to that of the Aldabra tortoise: it is also a threatened insular giant tortoise of which both the generic and specific names were changed. In some other cases a single species appears under different names on different lists: for example, the bird mentioned on the CITES list as *Houbaropsis bengalensis* (Gmelin, 1789) appears on the IUCN list as *Eupodotis bengalensis*. We know of no evidence that these changes caused any problem for the conservation policy of these species.

Of course, we agree with Vences (BZN 66: 282, September 2009) that, for conservation (but also other) purposes, stabilising the nomenclature of the Aldabra tortoise will ultimately be important, and that, now that Frazier has challenged the correct nomenclature, it is unlikely that the scientific community will reach a consensus on which name to use without an unambiguous decision of the Commission. But this does not imply in the least that this decision should follow Frazier's suggestion. The merits of Frazier's proposal have to be evaluated in the light of the next two arguments.

Nomenclatural stability

Among the 85 persons who expressed their support for the use of the name *T. gigantea*, 59 (69.4 %) did not challenge the respective identifications of the lectotype of *T. dussumieri* presented by Bour (1984) or of the holotype of *T. gigantea* presented by Pritchard (1986) and Bour (2006). Their opinion rested on the assumption that

usage of *T. gigantea* is sufficiently well established to require conservation of this name through use of the plenary power of the Commission, whatever species its holotype represents. However, comments by Bour & Pritchard and Cheke (BZN 66: 174–176, June 2009) disagreed with this assumption. The statement by Frazier regarding usage of the name *gigantea* was clearly demonstrated to be in error by Bour, Pritchard & Iverson (BZN 67: 73–77, March 2010), through a survey of Google. In recent years, this single species has been designated mostly under 3 different specific names, *dussumieri*, *elephantina* and *gigantea*, and 3 different generic names, *Aldabrachelys*, *Dipsochelys* and *Geochelone*. These data show: (1) that no universality of usage exists regarding these names; (2) that the relative numbers of authors using each of these names are constantly changing; and (3) that, during recent months, the relative frequency of usage has increased for *gigantea* and *Aldabrachelys*, but decreased for *dussumieri* and *Dipsochelys*. As a matter of fact, the figures obtained about nine months after publication of Frazier's application show a strong impact of this application itself upon usage. In addition to the content of the application itself and personal contacts of its author with colleagues, this is clearly due to (1) the fact that the title of this application contains the misleading statement 'currently *Geochelone (Aldabrachelys) gigantea*', and (2) the fact that the *Code* states that while such a case is under consideration the invalid name (under the normal rules) has to be used! On the last check-list of the extant turtles and tortoises by Rhodin et al. (2009), contrary to that by Fritz & Havas (2007), both combinations *Aldabrachelys gigantea* and *Dipsochelys dussumieri* are proposed together as an alternative to name the Aldabra tortoise. Pending the decision of the Commission, this appears to us the best attitude to adopt in the present highly controversial situation.

The arguments of the supporters of Frazier's application are mostly directed against the name *dussumieri*, but this is a biased presentation of the facts. The name *dussumieri* was resurrected as the valid one for the Aldabra tortoise only in 1995 (Gerlach & Canning, 1995; Gerlach, 1997), but the fact that the name *gigantea* does not apply to the Aldabra tortoise had been established 13 years earlier, by Bour (1982). Bour had made an error concerning the biological species to which the holotype of *gigantea* belonged, an error corrected by Pritchard (1986) twenty years before the rediscovery of the holotype specimen by Bour (2006), but nevertheless it has been clear from 1982 that the name *gigantea*, created for a tortoise from Brazil, does not apply to the Aldabra tortoise. Therefore, between 1982 and 1995, pending the resurrection of the name *dussumieri*, it was normal and correct to use the name *elephantina* for this species, which explains why 18.5 % of the Google hits obtained by Bour, Pritchard & Iverson (BZN 67: 73–77, March 2010) concern this name. The Seychelles Island Foundation (SIF), some twenty members of which signed comments in support of the use of *Geochelone gigantea*, still recently used official documents where the Aldabra tortoise was named either *Dipsochelys dussumieri* or *Testudo elephantina* (e.g. Beaver & Gerlach, 1998; Anonymous, 2001). Furthermore, and contrary to the statements of Frazier and his supporters, the name *elephantina* had also been used a long time prior to 1982 by some authors, and there was no period in history when the name *gigantea* was the only one used for the Aldabra tortoise. During the so-called period of 'universality' of usage of the name *gigantea*, the name *elephantina* was regularly used as valid for a species or subspecies by a

minority of authors: e.g. Fritsch, 1871; Günther, 1877; Peters, 1882; G.A. Boulenger, 1889; Strauch, 1890; Schacht, 1903 (as *Testudo elephantina*); E.G. Boulenger, 1914; Geiman & Wichterman, 1937; Evans & Quaranta, 1949; Quaranta & Evans, 1949; Urbain et al., 1951; Wermuth & Mertens, 1961; Georg et al., 1962; Steers, 1968.

The *Glossary of the Code* (p. 121) defines 'prevailing usage' of a name as follows: 'that usage of the name which is adopted by at least a substantial majority of the most recent authors concerned with the relevant taxon, irrespective of how long ago their work was published'. This definition is not clear, as it does not provide guidelines to recognise 'the most recent authors' and 'at least a substantial majority', but the minimum that the latter words may mean is that this is a majority of 'much more than 50 %'. It is therefore clear that the argument of 'usage' does not hold in the present case. There has never existed any 'universality of usage' for the scientific name of the Aldabra tortoise, but a diversity of usages, and stabilisation of usage in this case should come from simply following the *Code*. If 'usage' of a specific name in 46.3 % of recent publications and internet documents, including many posterior to an application urging the Commission to stabilise this usage, against 35.3 and 18.5 % for two other names (see Bour, Pritchard & Iverson, 2010), could be retained to nullify the rules of zoological nomenclature, then we might as well get rid of these rules altogether, including the Principle of Priority and, why not, the entire *Code* itself, leaving so-called 'consensus' to decide upon the valid names of taxa.

Identification of the holotypes

Nineteen (22.4 %) of the 85 supporters of the name *T. gigantea* challenged the identification of the holotype of *T. gigantea* and one (1.2 %) did so for the lectotype of *T. dussumieri*. Because giant insular tortoises tend to share many homoplasies due to similar environmental conditions, we consider that the specific identification of museum specimens can be relied upon only if carried out by experienced taxonomists well-acquainted with the group of modern land tortoises. This is stressed by a droll example: one month after publication in this Bulletin of his support for the original application regarding this case, a museum curator sent the Paris Museum for identification a set of photos of a specimen of giant land tortoise, stating that he was 'not too sure' about what this specimen could be. Well, this specimen happened to be unquestionably . . . an Aldabra tortoise!

Among the 91 persons who commented on this case, only three have personally examined the holotypes of the two nominal species here at stake. The lectotype RMNH 3231 of *Testudo dussumieri* Gray, 1831 has been examined only by Hoogmoed (then curator of the RMNH collection), Bour (1984) and Pritchard (during a visit to the Leiden Museum in 2000), and the holotype MNHN 9566 of *Testudo gigantea* Schweigger, 1812 has been examined so far only by Bour (2006), although Pritchard has examined an extensive set of colour photographs of this specimen, sent to him by Bour. All other authors who commented on the taxonomic allocation of these specimens did so only on the basis of the original descriptions (which they presumably read) or possibly of published photographs of the holotypes, if they indeed had access to their publication in a little known journal (Bour, 2006). None of them ever wrote to our Museum to request access to the holotype of *T. gigantea*, or to photographs, radiographs or measurements of it, and we note with some regret that perhaps even the assumption that the contributors to this discussion

read Schweigger's original work, as well as those of Pritchard and Bour, may be in error for some of them.

Among the 16 testudinid taxonomists involved in the discussion beside Bour, 5 expressly mentioned that they agreed with Bour's (2006) statement that the specimen MNHN 9566 is indeed the specimen that had been described by Schweigger (1812), 6 did not question this statement, and 5 (Arnold, Fritz, Meylan, Parham & Rhodin) expressed scepticism about it, but did not identify a single morphological or anatomical character of this specimen that was not compatible with the original description. Another taxonomist (Matyot) questioned the origin of the holotype of *T. dussumieri*, but did not provide a reasonable argument to back up this opinion (see Bour, Iverson & Pritchard, 2010).

Finally, none of these supporters of Frazier's application provided any argument to explain the discrepancies between the original description of *Testudo gigantea* by Schweigger (1812) and the biological species of the Aldabra tortoise. Therefore, following the precise conditions put by Article 75.3 of the *Code* for allowing a neotype designation, it is clear that, as stated by Bour & Pritchard, Cheke, Gerlach and Hoogmoed, the designation of a neotype for this species by Frazier (2006) is invalid, as this specimen does not come from the original type locality (Brazil) and differs in several important characters (see the comments by Bour & Pritchard and by Gerlach) from the original description. Nobody in the world can agree that a specimen from Aldabra qualifies as coming 'as nearly as practicable from the original type locality' (Article 75.3.6) of a tortoise from Brazil. Therefore, even if the holotype of this nominal species had not been rediscovered, this designation would be null and void, and should have been replaced by another one based on a tortoise specimen from Brazil or, if this had turned impracticable (e.g. because of restrictive laws on the export of specimens of this endangered species), from a neighbouring country.

Therefore, we concur with Bour & Pritchard, Cheke, Gerlach, Hoogmoed and Iverson that no evidence has been provided by the supporters of the application that Bour's (1984, 2006) taxonomic interpretations of the holotype of *T. gigantea* and of the lectotype of *T. dussumieri* are incorrect. These data dictate that the Commission should not use its plenary power to set aside all previous type fixations, validate Frazier's neotype for the former, and suppress the latter.

Discussion

As present or past curators of an important natural history museum collection, we are quite worried about the turn that this discussion has taken. Most of its actors based their comments on opinions, tastes, or just 'deep feelings', rather than on the respective merits of rational arguments. In particular, we were very shocked to see the publication of personal attacks against our colleague Roger Bour, questioning his honesty and suggesting that he manipulated scientific data. These published statements will remain available in the literature long after the death of all contributors to this discussion. We have known Roger for about 40 years and we would like to praise his scientific competence, his intellectual honesty and his refusal to make 'political calculations' to allow his opinions to win or to enhance his career. He has devoted a lot of his professional life to clarifying difficult taxonomic and nomenclatural problems in chelonians, identifying old 'forgotten' types in many museums worldwide, and thus permitting genuine nomenclatural stabilisation based on scientific data, not on 'impressions',

‘profound hunches’ or ‘lobbies’. We understand these repeated attacks as a disapproval and a denial of the quality of his work, and we think he deserves better treatment from the part of the many colleagues worldwide who benefited from his help in their research for decades. We consider this failure as a pernicious result of the opening of this unwarranted case in this Bulletin, which is itself a consequence of the emphasis put by the Commission in recent years on ‘usage’ against the rules of the *Code*.

We agree with Hoogmoed’s statement that ‘*Frazier’s proposal is completely unnecessary, because the facts are clear and the rules of the [Code] provide solutions for this situation*’. The case at stake here in fact concerns neither conservation biology nor nomenclatural stability, but simply the accuracy of taxonomic work.

The doubts cast by some authors about the rediscovery of a holotype in an old collection like that of the Paris Museum demonstrate a poor knowledge about such historical collections. As experienced taxonomists, we had on various occasions the opportunity to rediscover specimens that were not labelled as types, not only in the Paris Museum but also in other old historically important collections, like those of London or Berlin. An important part of the herpetological collection of the Paris Museum, but even more so of the mammal and bird collections of that institution, is composed of historical specimens, many of which can potentially be name-bearing types, or at least vouchers of specimens mentioned in ancient publications. Such publications date from the end of the 18th and the 19th century, when no such regulations as the *Code* existed and no proper labelling of specimens as ‘types’ could be done, as such a concept did not exist or was used in a very vague way (e.g. an author could then decide to replace the original ‘type’ by a ‘more appropriate’ specimen). In a work in progress, the type catalogue of hyloid frogs in the Paris Museum herpetological collection (Ohler et al., in preparation) covers 156 names; 67 (43 %) were created in works published before 1854, and 90 (58 %) before 1900, at the time of implementation of the first *Code* (the ‘*Règles*’). A similar proportion of old names would probably be found in other parts of the collection, or this proportion may even be higher for a well-studied group like the chelonians. Some of the intervenors in this debate seemed to consider that it would be a crime of lese-majesty to consider that Duméril & Bibron, ‘these two doyens of herpetology’ (Lenin & Frazier, 2009) could have made an identification error on a specimen. However, everybody can make a mistake and, considering the monumental work they produced, there is nothing surprising or shocking to note that Duméril & Bibron (1834–1854) made a number of mistakes, not only about the identity of the holotype of *Testudo gigantea* (e.g. Shea, 2001; Lescure & Ohler, in preparation).

Old museum collections covering the whole of zoology still harbour thousands of historical specimens, including name-bearing types that have not yet been identified as such. Every thorough survey of old specimens of any zoological group in such museums is an adventure which can be as exciting as field work in the remotest places of the earth. Doing so, one sometimes finds unexpected results, e.g. regarding the taxonomic identification of old name-bearing types, and some names have to change. The increased availability and application of DNA sequencing technology to the proper taxonomic identification of name-bearing types promises that the frequency of nomenclatural complications will only increase. Should curators and taxonomists stop exploring these resources to avoid such unexpected findings? Should they throw away these old specimens for fear that they would upset ‘usage’ of the names at stake and then threaten

the personal comfort of some persons long involved in researches dealing with these animals? Should we just close museums in order to please ‘conservationists’ (of names)?

Bouchet (BZN 66: 77, March 2009) rightly stressed that ‘Vertebrate paleontology survived the name *Brontosaurus* giving way to *Apatosaurus*’, and it can be quite safely added that the extinction of these animals was not caused by this synonymisation. All palaeontologists now use the name *Apatosaurus* for this genus, and this change did not create problems for non-taxonomists, who may still use the common English name ‘brontosaurus’ for these animals. Whatever scientific name will ultimately be retained for this species, the Aldabra tortoise is and will remain designated under its common name in many ‘non-taxonomic’ texts, including ‘conservation biology’ documents. Although different scientific names have been used for this species in the recent years, it has been clear to all involved that all those names designated the Aldabra tortoise and stabilisation of its scientific name will be an easy task as soon as the Commission has made its decision.

Conclusion

In conclusion, we urge the Commission to refrain from using its plenary power to suppress the holotype of *Testudo gigantea* Schweigger, 1812 or the name *Testudo dussumieri* Gray, 1831, and to simply place both these names, as defined by their name-bearing type specimens (respectively the holotype MNHN 9554 and the lectotype RMNH 3231), as well as the generic names *Aldabrachelys* Loveridge & Williams, 1957 and *Dipsochelys* Bour, 1982, as defined by their type species (respectively *Testudo gigantea* Schweigger, 1812 and *Testudo elephantina* Duméril & Bibron, 1835) on the Official Lists of Specific and Generic Names in Zoology.

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Having conducted systematic research on reptiles in the Indian Ocean for almost 25 years, I (and many others) have become increasingly frustrated by the ongoing nomenclatural confusion associated with the Aldabra tortoise. And now, it is remarkable to find 58 pages of Volume 66 of the *Bulletin of Zoological Nomenclature* being devoted to this one topic. This is a striking example where a ruling from the Commission is now urgently needed concerning Jack Frazier's Case 3463, which proposes to conserve the species name of the Aldabra tortoise as *gigantea*.

While I am sympathetic to the arguments made by colleagues that the holotype of *Testudo gigantea* Schweigger, 1812 is actually a *Chelonoidis denticulata* (Linnaeus, 1766) collected from Brazil, unfortunately, the rediscovery of this long lost holotype has not been universally accepted by the systematic community (see earlier comments on this case). Further, there is now new doubt that the lectotype of *Testudo dussumieri* Gray 1831 was collected from Aldabra (Matyot, BZN 66: 352–354, September, 2009).

The best solution for dealing with all this uncertainty is the designation of a neotype that clearly originates from Aldabra, and conserving the species name that has the broadest current recognition and usage. Jack Frazier's Case 3463 proposes that *Testudo gigantea* Schweigger, 1812 is conserved to stabilize the nomenclature of the Aldabra tortoise, and that the specimen USNM 269962 collected from Aldabra, is designated as the neotype for this species. I support the designation of this neotype with well-documented provenance from Aldabra and, based on the published comments presented in response to this case, it is clear that there is strongest support for conserving the species name *gigantea*.

Comments on the proposed conservation of usage of *Archaeopteryx lithographica* von Meyer, 1861 (Aves) by designation of a neotype.

(Case 3390; see BZN 64: 182–184, 261–262; 65: 314–317; 66: 87–88, 357–358)

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Padian's (BZN 66: 357–358) suggestion that the isolated fossil feather described by von Meyer (1861a) is a satisfactory holotype for the nominal species *Archaeopteryx lithographica* deserves to be rejected for several reasons. All of these points had already been addressed in the original application and/or in the published comments, but as they have been passed over without discussion, a recap seems to be necessary:

(1) The suggestion that the single feather can be unequivocally identified with the skeleton finds made to date is based on the premise that only one species of feather-bearing animals ever lived at the discovery localities and their surroundings. The taxonomic judgement that the skeleton finds made to date belong to a single

species may well be correct, but for those authors who distinguished several species of fossil bird amongst these finds it is impossible to identify the name *Archaeopteryx lithographica* with any of the skeletons if the feather is the type specimen. Further, it cannot be predicted with absolute certainty that another species of feathered animal will never be found at Solnhofen or its vicinity. In this case, again, the ‘holotype’ feather may turn out to be useless in deciding to which species the name *Archaeopteryx lithographica* actually belongs.

(2) It is good practice to base a taxon name, particularly a name so well known as *Archaeopteryx lithographica*, on a specimen which actually shows a large number of the characters defining the taxon. The London specimen is the only one to be considered for this purpose, as it was (contrary to Padian’s statement) originally associated by von Meyer (1861b and thereafter) with the name *Archaeopteryx lithographica*, albeit in a very ambiguous manner.

(3) According to Padian, ‘there is no reasonable disagreement that the name was applied to the feather [. . .] There is a possibility that von Meyer later intended the name also to apply to the first discovered skeletal specimen, although this is ambiguous and in any case irrelevant because the referral to the skeleton was secondary.’ Apparently Padian did not notice the analysis of von Meyer’s (1861a, b and 1862a) papers published in 2008 (BZN 65: 314–317). Von Meyer’s statements (1861b): ‘[. . .] I received news [. . .] that an almost complete skeleton of an animal covered in feathers had been found in the lithographic slate. [. . .] For the denomination of the animal I consider the term *Archaeopteryx lithographica* as appropriate’ shows that (a) he did not consider the feather as representative of the nominal species *Archaeopteryx lithographica*, (b) he provided no definition, however short or incomplete, of the ‘animal’, (c) he did not make an unequivocal statement whether he thought the feather belonged to the ‘animal’ called *Archaeopteryx lithographica*. Von Meyer’s wording suggests that the skeleton (the London specimen) could represent the animal which produced the feather, but does not actually state it, doubtless because he knew nothing about the skeleton. It is obvious that von Meyer intended to preempt the naming of this important discovery before he had any relevant knowledge of it.

In accordance with the foregoing, I maintain support for the original application of Bock & Bühler (BZN 64: 182–184) with the additions by Kadolsky (BZN 65: 314–317).

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I wrote previously in support of Bock & Bühler’s proposal to designate a neotype (BMNH 37001 in the Natural History Museum, London) for *Archaeopteryx lithographica*. In a recent comment, Padian (BZN 66: 357–358) expressed an opposite view. I would like to add a further comment to address these criticisms.

Padian might be correct in arguing that ‘there is no convincing evidence that the feather and the ten skeletal specimens do not belong to the same taxon’; however,

it is equally fair to say that there is currently no convincing evidence to demonstrate that they do belong to the same taxon (Elzanowski, 2002). How can we determine this? The core issue is, are we talking about the differences between various skeletons or the difference between the single feather and the skeletal specimens? If it is not even certain that the single feather belongs to the same taxon as the skeletons, or even to a bird (given the presence of feathers in other non-avian dinosaurs), then I would strongly suggest it is time now to consider a neotype for *Archaeopteryx* in order to discuss how many taxa actually existed among archaeopterids.

It should be pointed out that Padian's argument that 'no non-archaeopterygid theropod from the Solnhofen limestones is known to have possessed feathers' implies an assumption: the feather must be the same taxon as the skeletons of archaeopterygids, thus his conclusion is circular.

I also disagree with Padian that 'if a neotype were to be designated, it should be based on the best available skeletal specimen, regardless of the order of discovery'. To me, the criterion for choosing a neotype should follow the most important principles in taxonomy, i.e., convention and priority. BMNH 37001 represents the first reported skeletal specimen of *A. lithographica* that was discovered nearly as early as the feather. It has also been much more extensively studied and is possibly more commonly associated with the name than the feather and any other skeletons (see previous comment by Barrett and Milner (BZN 64: 261–262)).

Padian also commented on the feathers from the Jehol Biota. I agree that the presence of feathers in Early Cretaceous birds in China is irrelevant to this case. Yet, the presence of feathers or protofeathers in a number of non-avian theropod taxa including the COMPSOGNATHIDAE, which was also found at Solnhofen limestones, is probably not irrelevant. Padian's conclusion that 'they represent a variety of theropod taxa not found in the Solnhofen limestones, none of which belong to ARCHAEOPTERYGIDAE', is true but is clearly based on comparison of the Chinese feathered theropods with the skeletal specimens of ARCHAEOPTERYGIDAE, not the holotype feather. Furthermore, feathered dinosaurs are now not only known from the Early Cretaceous Jehol Biota but also from Middle to Late Jurassic biotas (Zhang et al., 2008; Hu et al., 2009).

Contrary to Padian, I believe there are at least two important reasons to designate a neotype for *Archaeopteryx*: firstly, there is at least a possibility that there are more than one species of *Archaeopteryx* or archaeopterid in the Solnhofen fauna, and the holotype does not preserve sufficient anatomical information to be referred to any of these potentially valid taxa with confidence; secondly, feathers of modern appearance are now known, without doubt, from non-avian dinosaurs whose stratigraphical range spans 160 Ma to 120 Ma. Furthermore, even if the holotype feather does indeed belong to a bird, there is no guarantee that feathers will not be found in non-avian dinosaurs at Solnhofen in the future.

For many reasons, I do not see any advantage in retaining a single feather that contains almost no taxonomic information as the holotype of such an important biological species, which is now known from several complete skeletons. Finally, I disagree with Padian's comment that 'the feather should remain the holotype until there is consensus that more than one species is represented in the fossil population at Solnhofen'. If the single feather remains as the holotype of *A. lithographica*, I

wonder how we can ever reach a consensus that more than one species of *Archaeopteryx* is represented in the fossil population at Solnhofen in addition to *A. lithographica*.

Additional references

- Elzanowski, A. 2002. Archaeopterygidae (Upper Jurassic of Germany). Pp. 129–159 in Chiappe, L.M. & Witmer, L.M. (Eds.), *Mesozoic birds: above the heads of dinosaurs*. University of California Press, Berkeley.
- Zhang, F., Zhou, Z., Xu, X., Wang, X. & Sullivan, C. 2008. A bizarre Jurassic maniraptoran from China with elongate ribbon-like feathers. *Nature*, **455**: 1105–1108.
- Hu, D., Hou, L., Zhang, L. & Xu, X. 2009. A pre-*Archaeopteryx* troodontid theropod from China with long feathers on the metatarsus. *Nature*, **461**: 640–643.

Comments on the proposed conservation of *Anthochaera* Vigors & Horsfield, 1827 and *Philesturnus* Geoffroy Saint-Hilaire, 1832 (Aves) by suppression of the generic name *Creadion* Vieillot, 1816

(Case 3499; see BZN 66: 332–339)

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We wish to comment on the proposed suppression of *Creadion* Vieillot, 1816, as one aspect of the submission as presented is contentious. While it may or may not affect the outcome, we would nevertheless like the Commission's opinion.

The contentious issue is the valid subsequent designation (or otherwise) of *Sturnus carunculatus* Gmelin, 1789, by Vigors & Horsfield (1827) in the (as admitted by Bock, Schodde & Palma) 'landmark review' of Australian birds. The original passage concerned, in full, is as follows:

'As we consider our present group [*Anthochæra*] to have no relation to the Sturnidae beyond the unimportant analogical one of having in common with two or three species carunculated appendages to the head, we have no hesitation in removing it [*Anthochæra*] from the genus *Creadion*; leaving the *Sturnus carunculatus* (with which species however we must confess that we are unacquainted) to represent that group.'

Bock, Schodde & Palma quite correctly state that Article 67.5 requires that the fixation of type species 'be rigidly construed', with which we are in perfect accord. Their citation, however, of Article 67.5.1, in relation to the word 'represent' in the above would appear to be less than satisfactory, as clearly generic limitations were being drawn by Vigors & Horsfield at this point, and the statement is no mere 'mention of a species as an example of a genus'. We would contend, therefore, that far from not qualifying, this does indeed constitute a valid type species

designation under Article 69.1.1, with 'represent', within this context, being an 'equivalent term'.

Neither can this be, moreover, an invalid type species fixation by elimination (Article 69.4) as then contended by the authors, as that proscribes the elimination of all but one of the originally included nominal species without, by implication, any statement concerning the fate of the remaining taxon, which clearly there is.

We would be grateful if the Commission would address the above concerns in their deliberations, and in coming to their conclusion regarding this case.

We have two final comments. Firstly, we agree with Bock, Schodde & Palma that the type species of *Anthochaera* was fixed by Gray (1840, p. 15). Secondly, we wish to point out that *Creadion* is neuter (transliterated from a Greek neuter noun) and that therefore uses of *Creadion carunculatum* and *C. rufusatrum* are all correct, as exemplified by Hœven (1852–1856, *Handbuch der Zoologie*, p. 510), Pelzeln (1873, *Archiv für Naturgeschichte*, p. 52), and Wolters (1980, *Die Vogelarten der Erde* Lieferung 6, p. 448).

(2) Colin Miskelly

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I agree with the case presented by Bock, Schodde & Palma, and support their recommendations to the Commission in full.

(3) Checklist Committee, Ornithological Society of New Zealand; B.J. Gill (convener), B.D. Bell, G.K. Chambers, D.G. Medway, R.P. Scofield, A.J.D. Tennyson & T.H. Worthy.

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The Checklist Committee of the Ornithological Society of New Zealand Inc. supports the proposal of Bock et al. (Case 3499) to suppress the name *Creadion*, which was in the past applied incorrectly to the saddlebacks (CALLAEIDAE) and is open to application to the genus presently known as *Anthochaera*. We support this action to promote stability of Australasian bird names, including continued use of the name *Philesturnus* for New Zealand's two species of saddlebacks. The Committee has in press a 4th edition of its checklist (OSNZ, 2010) in which *Philesturnus* will be used for the saddlebacks and the name *Anthochaera* for the Australian red wattlebird. We note that stability in bird names is as important as ever with the great current interest in birds for studies of molecular biology and the extensive use of scientific names of birds in DNA databases. (R.L. Palma is a member of the Checklist Committee but has excused himself from this letter as he is a co-author of Case 3499).

Additional reference

OSNZ (Ornithological Society of New Zealand) Checklist Committee. 2010 (in press). *Checklist of the birds of New Zealand, Norfolk and Macquarie Islands, and the Ross Dependency, Antarctica*. Ed. 4. Te Papa Press, Wellington, New Zealand.

Comments on the proposed precedence of *Procynosuchus* Broom, 1937 (Therapsida, Cynodontia) over *Cyrbasiodon* Broom, 1931 and *Parathrinaxodon* Parrington, 1936 (Case 3431; see BZN 66: 64–69, 188)

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I have been working on cynodonts for several years. The name *Cyrbasiodon* is long forgotten and only *Procynosuchus* is widely used by all known researchers. I support the use of *Procynosuchus* instead of *Cyrbasiodon*.

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This comment is sent for the purpose of offering complete support for the conservation of the generic name *Procynosuchus* Broom, 1937 (Case 3431) for a well-known group of basal non-mammalian cynodonts, instead of using the older, poorly known names *Cyrbasiodon* Broom, 1931 or *Parathrinaxodon* Broom, 1936. As someone who has worked on non-mammalian cynodonts, I can vouch that the name *Procynosuchus* is well established in the literature and publications on this taxon continue to appear on a regular basis. In contrast, *Cyrbasiodon* and *Parathrinaxodon* are obscure taxon names and have only rarely appeared in the literature since their inception. Formally recognising that *Procynosuchus* is a junior synonym of *Cyrbasiodon* or *Parathrinaxodon*, although following the Code, would cause extreme confusion, not only for those working in the field, but particularly for non-specialists such as science writers and textbook authors. I thus give my strong support for the conservation of the generic name *Procynosuchus* Broom, 1937.

Comment on the proposed conservation of usage of *Cuvieronius* Osborn, 1923 (Mammalia, Proboscidea)

(Case 3479; see BZN 66: 265–270)

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I agree with Lucas's arguments for establishing *Mastotherium hyodon* as the type species for *Cuvieronius*. The two original teeth described in Cuvier (1806) are difficult to place in two separate taxa, and probably represent one taxon. Also, it is impossible to distinguish isolated teeth of *Cuvieronius* and *Haplomastodon*. It is also apparent that the late 19th and early 20th century distinction of two separate species, *Mastodon andium* and *Mastodon humboldtii*, based on geography, was flawed. *Mastotherium hyodon* is indeed the oldest binomen, although Osborn (1923, 1926)

was the first to name a type species for *Cuvieronius*, using *Mastotherium humboldtii*. When Cabrera (1929) considered *Cordillerion* a junior synonym of *Cuvieronius* he erroneously chose *Mastotherium hyodon* as the type species for *Cuvieronius* instead of the correct *Mastodon humboldtii*. Subsequently most studies since then have used *Cuvieronius hyodon* as the type species. Although Ficcarelli et al. (1995) was correct in designating specimen MNHN TAR 1270 as the holotype for *C. tarijensis* since it was the first identifiable holotype, the long usage of *C. hyodon* merits some consideration. Given the confusion and complexity in the history of the nomenclature for *Cuvieronius* and the inadequate type specimen it makes sense to designate an acceptable neotype that exemplifies the genus. I believe that the original molar described by Cuvier (1806) and given the binomen *Mastotherium hyodon* by Fischer (1814) could still be assigned to *Cuvieronius* as geographical distribution should not weigh heavily on determining taxa. Thus, establishing *Mastotherium hyodon* as the type species, and MNHN TAR 1270 as a neotype is a plausible solution that will ensure stability. Consequently, *C. tarijensis* becomes a junior synonym.

Comment on the proposed conservation of usage of *Mastodon waringi* Holland, 1920 (currently *Haplomastodon waringi*; Mammalia, Proboscidea) by designation of a neotype

(Case 3480; see BZN 66: 164–167, 358–359)

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I agree with Spencer G. Lucas for the following reasons: a neotype needs to be established since the original specimens, from previous types (*M. chimborazi* and *M. waringi*), are now lost or lacking some of the original material. And, given the close relationship and similarities with other South American proboscideans, a neotype such as MECN 82, 83, 84, and 133 is needed. The skull and enamel-free, upward curving tusks of *Haplomastodon* with little or no spiralling are the main characteristics separating it from *Stegomastodon* and *Cuvieronius*. Although *Mastodon waringi* is the oldest name for specimens now referred to *Haplomastodon* (Holland, 1920), based on an incomplete jaw and tusk fragment (CM 11033), the current fragmentary nature of the specimen makes it problematic for distinguishing them from other genera. Hoffstetter (1950), believing Holland's specimen wasn't complete enough to warrant type status, designated *M. chimborazi* as type species of *Haplomastodon* based on a skull, which was subsequently lost in a fire.

Through this confusion *H. waringi* has been used as the type species of the genus by most workers since Hoffstetter. Although this is incorrect, as long as *H. chimborazi* is maintained as a junior subjective synonym it seems practical to continue using this long-used binomen, since it is the oldest and most widely used.